

present more detail, are brighter, and appear larger than when seen through an ordinary single telescope of the same aperture. In addition to this, the "seeing" is much steadier, and the stereoscopic effect obtained greatly enhances the beauty of the objects observed.

Such objects as Clark's companion to γ Lyrae, the companion to τ Orionis and the Mitchell companion to Rigel have all been steadily observed, and it is generally considered necessary to use an instrument of 7 or 8 inches aperture in order to see the last-named object.

The prisms used in this instrument are $2\frac{1}{2}$ inches long and $1\frac{1}{8}$ inches thick, the rays from the objectives traversing $5\frac{1}{2}$ inches of glass before reaching the eyepieces.

RECENT AMERICAN BOTANY.

MR. M. L. FERNALD¹ has published a very interesting review of the birches belonging to the groups *Betula alba* and *B. nana*. These trees and shrubs inhabit the northern regions of both hemispheres, and Mr. Fernald recognises in America seven species and seven varieties, of which six species and five varieties are common to the Old World. Thus, contrary to the opinion of some recent authors, the American white birches are mostly non-endemic, though exhibiting numerous apparently distinct forms. Not only is this true, but the admitted species intergrade all along the line. "It is quite possible to trace by a series of specimens a direct connection between the dwarf *Betula nana* or *B. glandulosa* and the tall *B. alba*. . . . But since it is obviously impracticable to regard all these forms as one species, it seems wiser to recognise the more marked centres of variation as species which are admitted to pass by exceptional tendencies to other forms ordinarily distinguished by marked characteristics" (p. 189). This, of course, brings up the question of the definition of species. The present writer has been accustomed to use the accompanying diagram in teaching biology. The line *a a* represents a species which is slightly dimorphic, as is indicated by the two prominences. The line *b b* represents a strongly dimorphic species, connected (at *b'*) by very few intermediates. The line *c c* represents a case in which the intermediates have died out, and there is a complete break (at *c'*) resulting in the formation of two species. It is now to be pointed out that this break must be spacial or geographical, and not merely morphological, otherwise the two sexes of the same species would often have to be regarded as distinct species. Such a break need not be geographical in the ordinary sense, but when the two species inhabit what is nominally the same locality, they are found to be differently related to their environment, or related to different closely adjacent environments. Furthermore, they must breed true, and not ordinarily interbreed one with another.

This sounds simple enough, but the application of these principles is not so simple. In the diagram, the case of *b b* is obviously more like that of *c c* than it is like that of *a a*. The difference between a slight break and a slight connection is infinitesimally small, yet after all it is a real difference—something existing in Nature, and not subject to individual opinion. If this criterion is admitted, because of its capability of exact definition, then the whole series of birches discussed by Mr. Fernald must apparently be regarded as one species!

Another sort of case is offered by the plants of the Galapagos Islands, recently reviewed in a most valuable memoir by Dr. B. L. Robinson.² *Euphorbia viminea*, J. D. Hooker, has eight distinct forms confined to as many islands (one only being found on two). These plants are readily distinguishable, but their characters are such as would be ordinarily of no value for distinguishing species in the genus. On continental areas, similar species of *Euphorbia* are polymorphic, with innumerable similar variations connected by every sort of intermediate. Consequently, Dr. Robinson does not treat the Galapagos plants as separate species, or (with one exception) even as varieties, but as "forms." Now, according to the above definition of species, these plants are perfectly good species, for the breaks in continuity, slight as they are, appear to be absolute.

There is, perhaps, one way of escaping from this conclusion. Distinct species should not promiscuously interbreed; there should be some sort of "physiological" barrier. It is known, in the case of the ostensibly distinct species of *Lavatera* from the

islands off the coast of California, that this barrier does not exist. Perhaps, if the different Galapagos Islands forms of *Euphorbia viminea* were grown together, they would completely fuse and give a single promiscuously varying type like those of the continents. But, after all, the question is what they actually do, not what they might do, under hypothetical conditions. The answer to this question must be that they remain distinct.

It seems to the present writer that the only precise criterion of species must be a spacial one, just as the only reason for species is that of function, or the relation between the nature of the creature and the place it occupies. But, admitting this on philosophical grounds, we are forced to recognise species of every degree of distinctness, just as the geographer recognises islands separated by every sort of distance from the mainland. It is easier, no doubt, to accept instead the morphological criterion, and this is actually what we have to do in taxonomic work,¹ for lack of evidence of the other kind; but this leaves the whole matter to be decided by individual opinions, with results known too well.

It is probable, if not certain, that variable plants on continental areas produce many "temporary species." That is to say, local colonies become more or less differentiated and remain so until swamped by invasions of the parent form or some other variety. Whether we recognise these "temporary species" depends, in practice, upon the degree of difference exhibited. Not rarely, the distinctions are constant and marked over a certain area, but the very same distinctions elsewhere occur as individual variations in the midst of the parent species. I have recorded such cases in the genera *Sphaeralcea* and *Cleome*.

At the close of his work on the Galapagos flora, Dr. Robinson presents a most lucid and philosophical discussion of the whole subject; it is so full of fact and thought that a brief summary could not do it justice. In particular, attention must be called

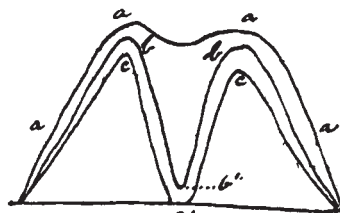


FIG. 1.

to his statement of the reasons why the local insular varieties persist in spite of the occasional infusion of new blood.

Mr. Carl Purdy's revision of the genus *Calochortus*² is another work of great interest. These beautiful "butterfly lilies" are extremely abundant in the Pacific region of North America, and are almost indefinitely variable. The variations are of all sorts, sometimes "constitutional" rather than morphological. Says Mr. Purdy, "In cultivation it has frequently been found that a very slight variability in strains is accompanied by a marked constitutional difference. In two beds of *Calochortus venustus*, planted in the same soil and separated only by a thin board, it would puzzle a botanist to state wherein the plants vary. They come from widely separated localities, and the difference is one more easily detected by the eye than conveyed by words. In one bed, two-thirds of the leaves are already destroyed by mildew (*Botrytis*), while in the other, not one leaf is injured; and such is the case whenever and wherever the two are planted" (p. 108). Mr. Purdy points out that in some localities the plants are very uniform, while in others they are extremely variable, with hundreds of distinguishable phases. It is probable that the phenomenon of "temporary species" is common in this genus, and the union of such morphologically, but not physiologically, distinct types is the cause of much variability. At the same time, there are species which always remain distinct, never producing fertile hybrids. That Mr. Purdy has tested so many of the forms for such "physiological barriers" gives his work especial value and importance. It does

¹ De Vries has assumed that, because botanists so distinguish species (admittedly of necessity), therefore the morphological criterion is the genuine one. Thus species have no better foundation in Nature than genera, which are wholly based on reasons of convenience.

² *Proc. Calif. Acad. Sci.*, 3rd series, Botany, vol. ii. No. 4 (1901).

¹ *Amer. Journ. Science*, xiv., September, 1902.

² *Proc. Amer. Acad.*, October, 1902 (vol. xxxviii.).

not appear that mere isolation suffices to produce even distinct varieties of *Calochortus*. For instance, *C. catalinae*, Watson, is found on Catalina and other islands, and also on the mainland; but instead of running into numerous insular races, it "is one of the least variable" of all, and no variety has been distinguished by name. On p. 141, Mr. Purdy admits that his *Calochortus venustus*, var. *eldorado*, "var. nov.," is the same as *C. venustus purpurascens*, Watson; while he applies the name *purpurascens* (Purdy, 1895) to a quite different variety of the coast range. This surely cannot be permitted; the former must stand as *purpurascens*, while the latter may be called var. *Caroli*.

T. D. A. COCKERELL.

EARTHQUAKE OBSERVATIONS IN GALICIA.

THE ninth number in the new series of the publications of the Austrian Academy of Sciences relates to earthquakes observed during the year 1901 in Lemberg. The first feature which one observes in this publication, the author of which is Dr. W. Láska, is that he describes each earthquake according to the phases it exhibits, the various phases being distinguished from each other by differences in their periods. Twenty years ago, earthquakes were described as consisting of preliminary tremors, shocks and concluding vibrations, each of which had distinguishing periodic motions. Now we find first preliminary tremors of types p_1' and p_1'' , second preliminary tremors of types p_2' , p_2'' , p_2''' and p_2'''' , and on they go, commencing with p_1' , with periods between 2.1 and 6.9 seconds, and ending with types where the periods have exceeded one minute. Inasmuch as these groups overlap, so that it is frequently difficult to assign a set of waves to their proper group, for our own part we are for the present content to divide the seismic spectrum into four parts—first and second preliminary tremors, large waves and concluding vibrations. In addition to these entries, Dr. Láska gives tables of tri-daily readings of two levels and of a thermometer. The most interesting portion of the work is, however, found in its introduction, where, amongst other matters, reference is made to the natural period of a pendulum as influencing the magnitude of its records and to rules which enable an observer to determine the distance of an origin from the inspection of a seismogram.

One simple rule is to diminish the duration of the first preliminary tremors reckoned in minutes by unity and multiply the same by 1000. The result is an approximation to the distance of the origin expressed in kilometres. For example, if a seismogram shows that the preliminary tremors had a duration of 7.6 minutes, then the earthquake it represents originated at some place about 6600 kilometres distant. The mnemonic is certainly simple, but its application is confined to those records where preliminary tremors are well defined. These are comparatively few in number and the accuracy of the determination is dependent upon the measurement of intervals of time which are small. These objections apply to a second rule suggested by Dr. Láska, the value of which is apparently still further impaired by the introduction of two assumed constants determined by Dr. F. Ömori. These constants are the velocities of the first and second preliminary tremors as determined from observations of ten earthquakes which originated near Japan and were recorded at Tokio and in Italy.¹ To obtain these velocities, the arcual distance between the Tokio isoseist and Italy is divided by the difference between the times of observation in Tokio and Italy. Had the distance between the origins and Italy been divided by the difference of times between the times of origin (which are easily calculable) and the times of arrival in Italy, then the constants given by Dr. Ömori would have been reduced. A further reduction would be made on the assumption that the wave paths of the motion considered had approximated to chords. If the speed of the preliminary tremors between their origin to the Tokio isoseist had been the same as it was from that isoseist to Italy, then the above objections might be withdrawn, but this, according to Dr. Ömori's own showing, appears hardly to be the case.²

Although it is interesting to find the relationship between the duration of preliminary tremors and the distance they have travelled again brought to our notice, the well-known method

of determining origins by the interval of time between the first motion of an earthquake and the subsequent arrival of the large waves is apparently one of more frequent and certain application.¹

J. MILNE.

PILOT CHARTS OF THE METEOROLOGICAL OFFICE.

IN addition to the usual information, the Meteorological Office pilot chart of the North Atlantic and Mediterranean for the month of January deals with some new features, necessitating the use of the back of the chart as well as the front. There is an account of the destructive cyclone which visited our coasts on October 15-16 last, and also of the slow-moving disturbance and its accompanying gales which wandered about the Tyrrhenian Sea from October 22-29. A summary is given of the characteristics of the surface temperature of the Atlantic for each of the ten months from January to October last, the most striking feature being the evidence of a distinct tendency for the water in the immediate vicinity of western Europe to remain cooler than the normal during the first nine months, a fact which may be associated with the persistent low air temperature over the adjacent land during the spring and summer. On the Newfoundland banks, there was a marked excess of warmth through the first six months, little or no ice being found in the locality. In October, an excess was shown on the eastern side of the ocean for the first time, and simultaneously the air temperature over the British Isles passed above the average in all districts. With the object of discovering what connection, if any, there is between the movements of weather systems and the distribution of the temperature of the surface water, observations are being collected for obtaining the mean barometric pressure month by month over the region from 30° to 60° N., 0° to 70° W., and the tracks of the centres of storm areas. For October, the mean isobars are superimposed on the sea temperature results, while the storm tracks are given on a separate chart.

To arrive at any definite conclusion as to cause and effect, it will require a long series of such charts—probably, too, for shorter periods than a calendar month, periods determined by the prevailing type of conditions, depending mainly on the positions and stability of the controlling anticyclones. Summaries are given of the ice reports from the whaling steamer *Balaena*, up Davis Strait, and the barque *Lady Head*, in Hudson Bay, last summer. Neither vessel passed any ice in the lower part of Davis Strait when heading for home in October. On July 1 last, the New Zealand Shipping Company's s.s. *Waikato* was disabled in 33° S., 6° E., and for twenty-six days she drifted helplessly about the south Atlantic, being finally taken in tow on July 27 in 28° S., 13° E., having in the interval travelled 812 miles, or at an average rate of more than thirty-one miles per day. The track of her wanderings day by day, together with the direction and force of the wind, supplied by Captain Kiddle, is reproduced, with the addition of the normal current circulation of the region, which shows that the *Waikato* followed closely the drift indicated by the Admiralty chart.

STARVING A PARASITE.

IN a recent paper read before the Royal Society,² Prof. Marshall Ward described the results of three series of experimental cultures of *Brome*-seedlings in sand, to which had been added various nutritive salts, or manurial mixtures, which were then infected with the parasite to see how the latter behaved on starved seedlings. Some of the seedlings received all the salts necessary for successful development, others none of such salts other than the root-hairs could extract from the sand itself and from the reserves in the endosperm, and others all necessary minerals except phosphorus, or potassium, or magnesium, or calcium, or nitrogen respectively.

So far as the seedlings themselves are concerned, the effects of the mineral starvation were most evident in the small stature,

¹ "Brit. Assoc. Reports," 1900, p. 79; and "Seismological Investigation Report," 1902.

² "Experiments on the Effect of Mineral Starvation on the Parasitism of the Uredine Fungus, *Puccinia dispersa*, on Species of *Bromus*." By Prof. H. Marshall Ward, F.R.S. Read before the Royal Society on November 27.

¹ "Publications of the Earthquake Investigation Committee in Foreign Languages," No. 5, pp. 71-80. (Tokio, 1901.)

² *Jour. Sc. Coll.*, Tokio, vol. xi, p. 158.